AN INQUIRY INTO THE *MODUS OPERANDI* OF MEDICINES UPON THE HUMAN BODY.

TO WHICH ARE ADDED,

SOME OBSERVATIONS ON THE ACTION OF THE LYMPHATICS.

By WILLIAM WYATT BIBB,
OF GEORGIA;
Member of the Philadelphia Medical and Chemical Societies.

"What miſchief have we done under the belief of falſe facts (if I may be allowed the expreſſion) and falſe theories!—We have aſſiſted in multiplying diſeaſes.—We have done more—we have encreaſed their mortality. I ſhall not pauſe to beg pardon of the faculty for acknowledging in this public manner the weakneſſes of our profeſſion. I am purſuing truth—and while I keep my eye fixed upon my guide, I am indifferent whither I am led provided ſhe is my leader."

RUSH'S INQUIRIES, vol. 1.

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AN

INAUGURAL DISSERTATION

FOR THE DEGREE OF

DOCTOR OF MEDICINE,

SUBMITTED TO THE EXAMINATION

OF THE

REV. JOHN EWING, S. S. T. P. PROVOST;

THE

TRUSTEES AND MEDICAL FACULTY

OF THE

UNIVERSITY OF PENNSYLVANIA,

ON THE

EIGHTH DAY OF JUNE, 1801.

TO

DOCTOR JOHN MURRAY,

OF

AUGUSTA, GEORGIA.

DEAR SIR,

TO paſs over in ſilence the numerous advantages I have derived from you, in the two-fold character of a friend and learned preceptor, when I have an opportunity of making a public acknowledgment, would be deviating too far from that propriety, which juſtice and gratitude preſcribe.

Permit me to declare that under your care I commenced my medical purſuits, and that your attention towards me, and exertions for my future proſperity, have been far greater than a pupil had a right to expect. Be aſſured, Sir, that the many favours you have conferred upon me ſhall ever be held in grateful remembrance.

Pleaſe accept a dedication of this my firſt eſſay as a ſmall, but ſincere mark of the high regard I entertain for your perſon and talents.

Wiſhing you the conſtant enjoyment of happineſs and proſperity through life,

I am Sir,

Your Sincere Friend,

And former Pupil,

WILLIAM WYATT BIBB.

TO

BENJAMIN RUSH, M. D.

Professor of the Institutes, Practice, and of Clinical Medicine,

IN THE UNIVERSITY OF PENNSYLVANIA.

DEAR SIR,

IN dedicating to you this imperfect Eſſay, I am influenced by a deſire to expreſs the high eſteem I entertain for your perſon and diſtinguiſhed talents; to declare that many of my medical principles I have the honour to derive from you; and, to return my ſincere thanks for the valuable inſtruction you have afforded me.

That your unrivalled abilities, and your exertions in the cauſe of humanity, may elevate you above the reach of the calumnies and unmerited perſecutions which you have ſo often ſuſtained, and that your eminent ſervices may obtain the permanent remembrance of a grateful people, is the ardent wiſh of

Your friend

And fellow citizen,

WILLIAM WYATT BIBB.

PREFACE.

Apologies, by way of preface, are so common, that it is with some reluctance I attempt to offer one. But duty to myself commands me to premise, that the intricacy of my present inquiry, the want of a pilot, and the short time allotted for writing an Inaugural Essay, combine with my very early youth and inexperience, to prevent the following pages from being so worthy public inspection, as I could wish. I am, however, compelled to obtrude them, claiming that indulgence which the preceding considerations may be thought to merit.

AN INQUIRY, &c.

THE ſubject, upon which I am about to enter, is one of the moſt comprehenſive, and intricate, I believe, in the ſcience of medicine. It is one, than which, in many of its eſſential relations, none can be of more importance, and, I may add, none is more involved in doubt and obſcurity.

In commencing this enquiry I am immediately met by a queſtion, to ſolve which in a ſatisfactory manner, I confeſs my inability—This acknowledgment, I preſume, will accord with that of every candid phyſician. The queſtion to which I allude is this—On what do medicines act when applied to the human body? We know they act upon the vital principle—but what is life, how formed, and how attached to the animal body, are queſtions, an explanatory ſolution of which is either reſerved for a more enlightened age, or will forever

be united with the important myſteries of nature.

Without then offering a diſcuſſion of the various opinions, relating to the form, action, and obſcure laws of life, I ſhall content myſelf with obſerving that it is the principle upon which impreſſions are made, that it is the ſource from which actions are derived, and that it is by it and ſtimuli, all the operations of the animal body are performed. I proceed then to enquire into the operation of medicines upon the human ſyſtem.

The human body is ſo conſtructed, that although there is an intimate connection between its various parts, although one ſound part often ſympathiſes with another that is diſeaſed, and although medicines frequently operate upon one part through the medium of another, yet we are not to infer that one of theſe parts cannot be affected by diſeaſe or medicine without the whole: Such an inference would be erroneous, as I hope to prove hereafter, inaſmuch as it is contradicted by the eſtabliſhed laws of diſeaſes, and the operation of medicines employed for their cure.

It is my intention in this eſſay to evince that all medicines act ſpecifically* upon one

* As the word *specific* is frequently employed in the fol-

or more parts of the body—that they are all ſtimulants†; and that their various effects which have induced writers to divide the materia medica in ſtimulants, tonics, aſtringents, &c. ariſe entirely from a ſtimulant *ſpecific* operation, exerted upon their appropriate parts of the animal ſyſtem.

Believing every diſeaſe, to which the body is ſubject, to have a particular ſeat, and every medicine to exert a *ſpecific* operation, I think myſelf ſupported by the laws of nature, in dividing the general ſyſtem into ſeveral leſſer ones. Directed then by the effects of diſeaſes, and the operation of medicines, I ſhall divide the body into ſix ſyſtems, viz. 1ſt. the Viſceral Syſtem, in which I include the ſtomach and inteſtines. 2d. The Sanguiferous Syſtem. 3d. The Nervous Syſtem, including the brain and nerves. 4th. The Muſcular. 5th. The Abſorbent, and 6th. The Glandular Syſtem.

lowing pages, and as it has often been uſed indefinitely, it is neceſſary I ſhould define what is meant by it. I mean then, that every medicine operates in a way peculiar to itſelf, and acts, in whatever manner applied, on particular parts chiefly.

† In aſſerting that all medicines are ſtimulants, I only include medicines properly ſo called. The abſtraction of ſtimuli from the body, is not conſidered as entitled to that appellation.

The relation which theſe different ſyſtems bear to each other is very unequal. The connection of the ſtomach with all of them is far more intimate than that which ſubſiſts between any others. The different parts of the body, therefore, are very liable to be affected, through the medium of this viſcus, both by diſeaſes and medicines, and vice verſa.

I am aware that the poſſibility of any medicine operating ſpecifically on one part, without exerting the ſame action in other parts, has been denied, and this denial is grounded upon the ſuppoſed ſameneſs in the operation of all medicines, and the peculiar ſympathy which connects the different component parts of the body. I acknowledge that a ſuperficial view of the organization of the whole ſyſtem, and of the uniform order of its various actions in a healthy ſtate, renders ſuch an opinion plauſible. But a more minute examination into the cauſes of thoſe actions, into the operation of the various ſtimuli which ſupport life, and into the effects of diſeaſes and medicines, upon this organized body, diſſipate at once this ſeeming plauſibility, and eſtabliſh on the firm baſis of fact and obſervation the propriety of the doctrine I advocate.

Such, indeed, is the independency of each ſyſtem, and ſuch the difference in the operation of the ſame cauſes upon them, that certain applications made to one, will be productive of fatal effects, which to another will be attended with mild and ſalutary conſequences. To exemplify this poſition it is only neceſſary to adduce the action of the carbonic acid. This, when inhaled into the lungs in form of gas, produces deleterious effects, yet it may be taken into the ſtomach, combined with water not only with ſafety, but with pleaſure. In this way it is ſo mild and agreeable, that many perſons uſe it in the heat of ſummer as a pleaſant diluting drink. I know it has been denied that carbonic acid gas, operates poſitively upon the lungs, and on the contrary aſſerted, that its fatal effects ariſe only by depriving them of atmoſpheric air. But this aſſertion is unſupported by facts and obſervation. Hiſtory informs us, ſays Chaptal, that two ſlaves whom Tiberius cauſed to deſcend into the Grotto Del Cano (which abounds with this gas) immediately expired. The ſame fate attended two criminals, whom Peter de Toledo ordered to be ſhut in there. Many ſimilar inſtances are upon record, of immediate death from carbonic acid in old

cellars and wells. It is ſaid that the famous lake of Averno, where Virgil placed the entrance of hell, prevents birds from flying over it with impunity, owing to the exhalation of this gas. The experiments of chemiſts fully prove its poſitive action. It is a notorious fact, that we can voluntarily ſtop breathing without much inconvenience, for a longer time than is required for this poiſon to produce its baneful conſequences.

The matter of ſiphilis which has done ſo much injury to mankind has been taken into the ſtomach without producing any of thoſe affections conſequent upon its application to its appropriate parts.

Dr. Fontana informs us that the poiſon of the viper is perfectly innocent when taken into the ſtomach, but when infuſed into a wound, produces the moſt terrific conſequences.

The foregoing are certainly examples not only of the ſpecific operation of medicines upon particular parts, but of their directly oppoſite effects; evincing in a deciſive manner the diſtinctneſs and independency of the different ſyſtems.

As the inveſtigation of this ſubject is not a matter of mere idle curioſity, but of the utmoſt practical importance, I ſhall purſue the

operation of medicines, with a view to ſhew their *ſpecific* action upon the different ſyſtems; and firſt

UPON THE VISCERAL SYSTEM.

Although I have divided the body into ſix ſyſtems, yet there are ſome medicines which operate principally upon only a part of a ſyſtem; an example of which is afforded by the ſtomach and bowels—thus the tartrite of antimony acts upon the former and jalap on the latter.

The unuſual ſympathy which I have declared to exiſt between the ſtomach and every part of the body; it being the medium through which they are acted upon by medicines, and often partaking of the diſeaſes of either ſyſtem, neceſſarily induces a belief that if medicines can operate ſpecifically upon this important viſcus, the ſame operation may attend their application to any other parts. That there are *ſpecifics* for the ſtomach is proved by the action of emetics, and that there are the ſame for the inteſtines is equally evinced by the operation of cathartics. If thoſe ſubſtances which induce vomiting or purging

when applied to the ſtomach or bowels, be applied even to diſtant parts they will produce the ſame effects. In proof of this aſſertion many facts may be collected. Mr. Sherwin rubbed four grains of tartrite of antimony into the palms of his hands, which in a ſhort time produced vomiting. The ſame effect has reſulted from injecting it into the blood veſſels. Mr. Smith injected a laxative medicine into the median vein of a patient in the hoſpital of Dantzic, which in four hours commenced a purgative operation continuing until the next day; and the celebrated Borrichius mentions that hellebore placed in an iſſue in the arm produced violent purging.

Thoſe ſubſtances which excite ſneezing when ſnuffed up the noſe, will produce the ſame effect if taken into the ſtomach. Profeſſor Barton informs us, in his lectures, that the honey which the bees prepare from the flowers of the kalmia latifolia, when taken into the ſtomach, cauſes ſneezing.

The powder of jalap or rhubarb when ſwallowed, paſſes through the ſtomach without much effect, but when they arrive to the bowels their choſen parts, they commence a powerful cathartic operation. This is not the caſe with ipecacuanha or the tartrite of

antimony which acts on the ſtomach, cauſing that violent exertion we term vomiting. How can this difference in the effects of theſe different ſubſtances be explained? and how ſhall we account for certain medicines affecting particular parts, although applied to diſtant ones?

If all medicines are ſtimulants, and their operation preciſely the ſame, varying only in degree, as is aſſerted by ſome writers, their effects on the ſyſtem ſhould alſo be the ſame, varying only in degree. But we have ſeen that different medicines do operate particularly upon different parts, and produce effects entirely different; therefore we muſt explain the fact in ſome other way.

This is eaſily and plainly done by admitting the evidence of truth which indubitably declares, that all medicines have a *ſpecific* action upon particular parts of the animal body.

In compliance with the opinion which ſuppoſes the only difference in medicines to conſiſt in the degree of ſtimulant property, every ſubſtance which would excite purging muſt produce vomiting in its paſſage through the ſtomach; becauſe this viſcus is endowed with more excitability or a greater aptitude to be

acted upon by ſtimuli than the inteſtines; therefore, thoſe medicines which produce any action in the latter, muſt excite much greater action in the former. The contrary of this, experience teaches us to be true, conſequently a *ſpecific* operation muſt be acknowledged.

Colombo, gentian, and bitters in general act ſpecifically upon the Viſceral Syſtem.—With a view to aſcertain the propriety of this poſition, I inſtituted the following experiments.

Experiment 1.

To my friend Mr. Mitchell, at 50 minutes after 9 o'clock, his pulſe beating 70 ſtrokes in a minute, I gave 20 grains of powdered gentian, mixed in a little milk. His pulſe was examined every five minutes attentively, for half an hour, without any variation being perceived. The ſame quantity was now repeated, and the examinations were continued with the ſame reſult, until 15 minutes had expired, when his pulſe beat only 67, in 5 minutes after 66, in 10—66, and in 15—64. The doſe was again repeated, in 5 minutes his pulſe beat 64, in 10—64, in 15—63, 20—63, 25—62, 30—62, 35—63, 40—64, 45—61, 50—58, 55—58 and in 60—60. The heat of his

body was a little increaſed, his pulſe appeared to be ſomewhat fuller, and he complained of a pain in his head.

Experiment 2.

To my friend Mr. Lockette, at 10 minutes after 10 o'clock, I gave 20 grains of powdered gentian as above, his pulſe beating 82 ſtrokes in a minute—in 5 minutes it beat 82, in 10 82, in 15—81, 20—78, 25—77, and in 30—76. The ſame quantity was again exhibited, in 5 minutes his pulſe beat 76, in 10—77, 15—77, 20—77, 25—73, 30—71, 35—70, 40—71, 45—73, 50—73, 55—73, and in 60—73. No other alteration in the pulſe was obſervable. He felt ſome degree of head-ach.

Experiment 3.

At 45 minutes after 9 o'clock, my friend Mr. Jackſon, his pulſe beating 78 ſtrokes in a minute, took 30 grains of powdered columbo, mixed in a little milk, ſome degree of nauſea enſued; in 5 minutes his pulſe beat only 68, and very weak, in 10—73, in 15—74, 20—75, 25—75, and in 30—77. The ſame quantity was now repeated, in 5 minutes his pulſe beat 73, in 10—73, 15—76, 20—76, 25—76, and

in 30—76. The ſame doſe was again taken, in 5 minutes it beat 73, in 10—75, 15—77, 20—74, 25—70, 30—71, 35—72, 40—70, 45—70, and in 50—70. In the latter part of the experiment, the pulſe was ſomewhat increaſed in force and fullneſs.

Experiment 4.

At 20 minutes after 10 o'clock, my friend Mr. Shepperdſon, took 30 grains of columbo as above; his pulſe beating 68 ſtrokes in a minute. In 5 minutes it beat only 63 and very weak; in 10—63, 15—61, 20---60 25—59, and in 30---59. The doſe was repeated. In 5 minutes his pulſe beat 59—in 10—59, 15—58, 20—57, 25---57, and in 30, 58. The ſame quantity was again taken ; in five minutes 58, in 10—58, 15—58, 20---60, 25—57, 30—57, 35—56, 40—55, 45---55, 50—54, 55—53, 60—53, 65—53, 70—53, 75—53, 80—55, 85—55, 90—55, 95---55, and in 100---55. His pulſe increaſed a little in fullneſs and force towards the latter part of the experiment, and he complained of a peculiar ſenſation in the ſtomach which he could not deſcribe.

The foregoing experiments fully evince 1ſt. That columbo and gentian do not ſtimu-

late the Sanguiferous Syſtem; therefore, that the generally received opinion reſpecting their operation is highly erroneous, and has had an injurious tendency in the practice of medicine.

2d. That they act *specifically* upon the Viſceral Syſtem, increaſing its action, and thereby leſſening the action of the heart and arteries---and

3d. That they may be exhibited with perfect ſafety in diſeaſes of the ſtomach and bowels, without any regard to the ſtate of the blood-veſſels.

From theſe facts I am induced to believe that beneficent nature has among her various productions, remedies peculiarly adapted to the cure of the diſeaſes of every ſyſtem of the body; and that medical ſcience has, and will derive its greatest advantages from the diſcovery of them, and the proper application of the principle of *specific* actions to the cure of diſeaſes.

II. THE SANGUIFEROUS SYSTEM.

Let us next examine whether there are not medicines which operate particularly upon this ſyſtem.

That the peruvian bark is a *ſpecific* for this ſyſtem, is rendered evident by its ſucceſsful application in the cure of its diſeaſes.

It may however be ſaid, that any medicine equally ſtimulating with the bark, will be equally efficacious in the cure of fever, and that its effects do not prove a *ſpecific* operation. This however, is far from being true, for opium which ſtands at the head of ſtimulants in the materia medica, is acknowledged by moſt phyſicians to be far leſs ſucceſsful. The bark may be employed in caſes with advantage in which opium would be productive of the worſt conſequences; the reaſon of which is, that the ſtimulant operation of the former is principally confined to the blood veſſels, while the latter acts as a potent ſtimulus upon the whole ſyſtem. It is by this general operation upon the whole body, producing that degree of debility in every part, which Doctor Ruſh has called the ſleeping point, that opium ſo much more uniformly induces ſleep than other medicines. The fol-

lowing experiment will evince the propriety of my opinion as to the operation of the peruvian bark.

To my obliging friend Mr. Little, at forty-five minutes after eleven o'clock, having breakfaſted at eight, I gave half a drachm of beſt red bark, mixed in a little milk, his pulſe beat ſeventy ſtrokes in a minute. I examined it every five minutes attentively until a quarter after twelve, without perceiving the ſmalleſt variation in force or frequency. I then gave him a drachm in the ſame form, repeating my examinations with the ſame reſult, until thirty-five minutes after the ſame hour, when it beat ſeventy one. The doſe was now repeated, in 5 minutes it beat 73, in 10—74, in 15—75, 20—76, 25—77, his face very much fluſhed, and he complained of pain in his head. I now gave him the ſame quantity as before, in 10 minutes his pulſe beat 78, and in 20—81. He now took the laſt doſe, in 10—85, and in 20—87, above which it did not riſe. There were no other perceptible effects produced, except a ſmall degree of nauſea.

The experiment now related tends to prove, 1ſt. That, the action of the peruvian bark is confined principally to the blood-veſſels, and that, for this ſyſtem it is a powerful *ſpecific*

ſtimulant; 2dly. That, the human body is ſo conſtructed, that one ſyſtem is capable of receiving powerful impreſſions even through the medium of other parts, without thoſe impreſſions being communicated to the whole; and 3dly. That, from the early commencement of its operation, after it was exhibited, it may produce its beneficial effects in the cure of fever through the medium of the ſtomach.

III. THE NERVOUS SYSTEM.

The operation of medicines, upon this ſyſtem, next merits our attention.

That every medicine has its appropriate ſyſtem or ſyſtems, I am clearly convinced. I ſay ſyſtems, becauſe ſome remedies act only on one, whilſt others act upon more. Among thoſe which operate upon this ſyſtem, the fœtid gums hold an eminent ſtation; aſſafœtida is one, the exhibition of which in nervous diſeaſes, has ever been attended with the moſt ſalutary effects. But alas! phyſicians not deducing proper concluſions from the experienced donations of nature---not viewing with propriety her beneficent operations, and forgetful that ſhe employs different means to ac-

compliſh her various purpoſes, have been led to conclude, that this medicine may be applied with equal advantage in the cure of the diſeaſes of the blood-veſſels---As well might we attempt to cure hypocondriaſis by blood-letting, becauſe it is efficacious in the removal of fever. Aſſafœtida has been recommended in the low ſtate of fever, when the moſt potent ſtimuli are required. And, thoſe who uſe it in theſe caſes, expect to obtain all the advantages of a powerful ſtimulus. That it is a powerful *specific* ſtimulus for the Nervous Syſtem, I agree, but that it has no perceptible operation upon the blood-veſſels, will be proved by the following experiment :

To the ſame friend on whom I performed my former experiment, I gave at half paſt eleven o'clock, ſix grains of aſſafœtida, in the form of a pill, his pulſe beat ſixty ſtrokes in a minute. The ſame quantity was repeated every half hour, until he had taken twenty-four grains, when after the ſame interval I gave him double the quantity.

The whole amounted to thirty-ſix grains in the ſpace of two hours, which is more I believe than is uſually exhibited in caſes of diſeaſe. His pulſe was examined every five minutes attentively, but without perceiving

the leaſt variation either in force or frequency. The only perceptible effects were the following: he was much more lively than uſual, and felt his mind exhilirated, which was ſucceeded by nauſea and flatulency. The ſmell of the gum was evident in his urine the next morning.

This experiment plainly ſhews the extreme error into which phyſicians have fallen who recommend the fœtid gum for the cure of the diſeaſes of the Sanguiferous Syſtem. Such a practice originating from the ſuppoſed fameneſs in the operation of all ſtimulants, has certainly done much miſchief: becauſe, as I have ſhewn that aſſafœtida does not produce any perceptible effect upon the blood-veſſels, ſurely to truſt to it in diſeaſes of this ſyſtem is almoſt leaving the patient to the operations of nature. Here then we diſcover the immenſe advantage which muſt reſult to the ſcience of medicine from inveſtigating the action of medicines upon the ſeparate ſyſtems of the human body. When that time arrives, at which the ſubject ſhall be ſo far developed, that the diſeaſes of every ſyſtem ſhall have their appropriate remedies, then will epilepſy ceaſe to be incurable, then will all thoſe diſeaſes which now elude the ſkill of the medical

world, become curable, and then will the ſcience of medicine reign triumpant throughout the civilized nations of the earth.

IV. THE MUSCULAR SYSTEM.

This ſyſtem is ſo nearly allied to the nervous, that the majority of thoſe medicines which act particularly on one, exert alſo a *ſpecific* operation upon the other. I have, however, thought proper to divide them, from a belief that there are ſome medicines whoſe action is more confined to one than the other. The oil of amber, I conſider a *ſpecific* for the Muſcular Syſtem. This conſideration is corroborated by its ſucceſsful application in the cure of tetanus, and ſimilar diſeaſes. Dr. Ruſh obſerves, that he has uſed this medicine with obvious advantage in tetanus, and thinks it well adapted to the diſeaſes of the Muſcular Syſtem. Dr. Cullen admits the ſucceſsful exhibition of the "fœtids" in the cure of ſpaſmodic diſeaſes, but accounts for their operation in the following manner: "The ope-
"ration of theſe I take to be in this way,—
"that as all diſagreeable ſenſations are ſeda-
"tive, or means of weakening the energy of

"the brain, ſo I conceive that our fœtid me-
"dicines, by obviating or moderating the in-
"creaſed excitement which begins ſpaſmodic
"affections, may be the remedies of theſe."
It will be readily perceived, that the Doctor commences his explanation of the modus operandi of the fœtid medicines upon a foundation which has no exiſtence in truth; conſequently his concluſions muſt be involved in error. That muſcular diſeaſes often ariſe from, and are connected with morbid nervous affections, is true; but that they are not always the effect of morbid excitement in the brain, is proved by the fact, that tetanus and convulſions are almoſt always preceded by, and connected with, the proper exerciſe of all the functions of the brain, the operations of the mind being perfectly unimpaired.

As to the Doctor's opinion of the ſedative action of the "fœtids," I deem it unneceſſary to ſpend any time in controverting it. I ſhall only obſerve that my experiment with the aſſafoetida, which I have related, evidently evinced a ſtimulant action. Our author has attributed the ſame ſedative operation to opium, and all the moſt potent ſtimulants in the materia medica; and, although he does this, we find him employing them in typhus fever,

and in all caſes of great debility, where ſtimulants are required; at the ſame time forbidding their uſe in inflammatory affections. Here we have a ſpecimen of the agreement between the theory and practice of the late Edinburgh Profeſſor.

As I have ſhewn that the fœtid gum operates as a powerful ſtimulant upon the Nervous Syſtem, and as I ſhall relate an experiment immediately, which proves the ſame operation of the oil of amber upon the muſcles, we muſt offer an explanation of their modus operandi in the cure of diſeaſes of theſe ſyſtems, very different from that of Dr. Cullen. I ſuppoſe then their good effects to ariſe from a *ſpecific* ſtimulant operation of each, exerted upon their appropriate ſyſtems, overcoming and deſtroying the morbid action, and producing a new and healthy one. With a view of aſcertaining the action of the oleum ſuccini upon the human body, I made the following experiment:

To my friend Mr. Jackſon, at eleven o'clock (his pulſe beating ſeventy-five ſtrokes in a minute) I gave ten drops of the rectified oil of amber mixed with a little ſugar. This doſe was repeated every fifteen minutes, until he took four times that quantity. His pulſe was

examined every five minutes attentively, without perceiving the ſmalleſt alteration until after the exhibition of the laſt, when it appeared to be a little fuller, without, however, any increaſe in quickneſs or frequency. After the ſame interval he took twenty drops, which were again repeated---his pulſe the ſame.--- In five minutes it beat 74 only, in 10—73, in 15—73, in 20—72, in 25—71, in 35—71, 40—71, 45—71, 50—72, and continued to riſe from this time until it arrived to its original ſtandard, but did not exceed it. The operation of the medicine upon the muſcles and brain was very perceptible. His mind was exhilirated, and he complained of langour or a painful difficulty in exerting his muſcles. At half paſt two, he walked out but returned in a ſhort time, finding the exercise of his muſcles unpleaſant.

I was aſſiſted in the foregoing experiment by a phyſician who believed the only difference in medicines to conſiſt in their degree of ſtimulant property. The reſult was as I have ſtated. Why did the pulſe diminiſh in frequency and increaſe in fullneſs---and why were the muſcles ſo debilitated? Theſe queſtions may be anſwered together, as I believe one to be dependant upon the other.---

The oil of amber exerted a powerful *specific* ſtimulant operation upon the Muſcular Syſtem, producing great indirect debility. This accounts for the painful exerciſe of the muſcles. The diminution in the frequency of the pulſe was cauſed by this ſtimulant operation on the Muſcular Syſtem, increaſing action there, and thereby leſſening the action of the heart and arteries. It may be aſked why increaſing action in one part ſhould diminiſh it in another? I anſwer, I know not, but ſuch is the fact; we muſt therefore admit it, and refer it to a law of the animal economy.

V. THE ABSORBENT SYSTEM.

This is ſo very important in the animal body, and a conſtant exerciſe of its functions ſo eſſential to health, that, to deny the exiſtence of *specific* remedies for its diſeaſes, would be accuſing nature of improper partiality.

The preparations of mercury and iron act powerfully upon this ſyſtem. Digitalis and ſquills may likewiſe be ſaid, with propriety, to operate upon the abſorbents. The juſtneſs of this opinion is fully evinced by their ſucceſsful application in the cure of anaſarca.

The great diſcharge of urine, accompanied with a ſudden diminution of the ſize of the abdomen, which ſometimes occurs in aſcites from the exhibition of digitalis, cannot be explained in any other way, but by admitting a *ſpecific* operation upon the abſorbents, cauſing them to abſorb the effuſed fluid, and upon the kidnies producing an increaſed ſecretion of urine.

It is well known that digitalis will leſſen the action of the pulſe, the concluſion from thence has been, that it operates powerfully upon the blood-veſſels; hence ſome have ſuppoſed it a ſedative, whilſt others declare that it proſtrates the action of the arteries by its powerful ſtimulant action upon them.

Both of theſe opinions I believe to be erroneous, as this medicine produces no action in the Sanguiferous Syſtem, but what ariſes from its *ſpecific* operation upon other parts. It acts ſpecifically upon the abſorbents and kidnies, increaſing their action, thereby leſſening the action of the blood-veſſels. Thus, morbid action in the ligaments may be overcome by exciting action in the ſkin by cantharides, or any other irritating application; mania and conſumption often cure each other. If nux vomica be given to a dog, and he be

whipped, it will not affect him. Many ſimilar inſtances, of action in one part deſtroying it in another, might be adduced, but I deem it unneceſſary.

It may with truth be obſerved, that thoſe ſyſtems of the body, the proper *ſpecific* medicines for which have been diſcovered, are far more eaſily cured of their diſeaſes, than thoſe the important remedies for which are yet unfortunately concealed. Thus, intermittent fever is nearly uniformly curable—Why? Becauſe, peruvian bark, a powerful *ſpecific* for the Sanguiferous Syſtem is diſcovered. Syphilis, alſo, is nearly always to be cured—and why? Becauſe, mercury, the deſtined *ſpecific* for the Glandular Syſtem, happily for mankind, is generally known. Nothing is wanting to render diſeaſes of the other ſyſtems equally curable, but equal progreſs in the diſcovery of their *ſpecifics*.

VI. THE GLANDULAR SYSTEM.

This has alſo its *specific* remedies. The Glandular Syſtem, however, is more frequently acted upon partially, by different ſtimuli, than any other of the body. It is ſo extenſive, and the different glands which compoſe it ſo diſtantly related, that many of them which are nearly connected by contiguity, or ſimilarity of ſtructure, are alone affected by particular medicines. Thus, digitalis and nitre operate upon the kidnies, yet they have no ſenſible effect upon the ſalivary glands or the liver.

Some of the preparations of antimony and ſulphur act upon the glands of the ſkin, yet they do not operate upon many other parts of this ſyſtem. The *specific* operation of cantharides upon the glands, on the internal ſurface of the neck of the bladder, is fully evinced by the fact that they will often affect them, although applied to the moſt diſtant parts of the body.—Marſh miaſmata exerts a *specific* operation upon the liver, though not upon other glands. Even mercury whoſe operation upon the glands is more general than any other medicine with which I am ac-

quainted, acts far more powerfully upon the ſalivary glands and liver, than upon the other parts of this ſyſtem. That, mercury operates ſpecifically upon the ſalivary glands, although it acts upon other parts of the body, cannot conſiſtently with truth, be denied. I know there are caſes upon record of other medicines ſalivating, but in theſe inſtances the action of the glands which ſecrete the ſaliva, is very different from that which mercury excites. Tobacco, and many other acrid ſtimulants when held in the mouth increaſe the ſalivary diſcharge, but this is not attended with that peculiar fœtor, ſore mouth, &c. which uniformly diſtinguiſhes the operation of mercury from that of every other medicine.

Ever ſince the diſcovery of the ſalutary effects of this mineral in the cure of ſyphilis, which is a diſeaſe of the Glandular Syſtem, phyſicians have been in conſtant controverſy relative to its modus operandi. To enumerate the various opinions which have been entertained upon the ſubject, and the arguments by which they have been ſupported, would require far greater limits than I have prefixed to this eſſay. And I confeſs I am by no means anxious to preſent ſuch a collec-

tion, as it would be only exhibiting a picture of the late degraded ſtate of medical ſcience. I therefore proceed to offer the opinion I have adopted upon the ſubject.

The matter of ſyphilis when taken into the human body, acts as a powerful ſtimulant, particularly upon the Glandular Syſtem, producing there its peculiar effects. That, mercury operates ſpecifically upon the ſame ſyſtem, cannot reaſonably be denied. Now, as no two actions can exiſt at one and the ſame time, in the ſame part, when mercury and ſyphilitic matter are both applied, one muſt ſucceed in deſtroying the action of the other. Experience teaches, that the former is moſt powerful, therefore eradicates the venereal action, by a ſuperior *specific* ſtimulant power, and either eſtabliſhes its own action which ſpeedily terminates in health, or immediately produces a new and healthy one.

It has been aſked why opium, ſtramonium and other medicines, generally conſidered far more ſtimulating than mercury, are not equally ſucceſsful in the cure of ſyphilis. To this I would reply, that although the operation of theſe medicines upon the whole body, is more powerful than that of mercury,

yet I am ſupported by the experience of phyſicians in aſſerting, that this latter has a more potent ſtimulant *ſpecific* action upon the individual Glandular Syſtem, which is the ſeat of diſeaſe, than any other medicine in the materia medica. I ſuppoſe its ſuperior efficacy, then, to conſiſt in the ſuperior property now mentioned.

That almoſt incurable diſeaſe called cancer is ſeated in this ſyſtem. Although mercury and cicuta operate ſo ſtrongly upon it, yet the cancerous action in moſt caſes is ſo great, that they cannot overcome it. It is, however, certain, that the moſt advantage is derived from the application of thoſe medicines, which act ſpecifically upon the Glandular Syſtem. Is there not a *ſpecific* ſufficiently powerful to overcome this fatal action? There certainly is: but until it is diſcovered, many of our unfortunate fellow-creatures muſt fall victims to its ravages.

I am ſupported then by truth, in deducing the following concluſions.

1ſt. The different diſeaſes of each ſyſtem are curable, or incurable, according to the progreſs which has been made in diſcovering their *ſpecifics*.

2d. The only method by which we can arrive at certainty, in curing diſeaſes, is, by attentively inveſtigating the operation of medicines upon the ſeparate ſyſtems of the body.

The doctrine of *ſpecifics* which I have delivered leads to the moſt important objects in the practice of medicine. 1ſt. To a proper ſelection of remedies for curing the diſeaſes of every ſyſtem, whereby we are capable of acting upon the diſeaſed part, without much affecting the healthy ones. 2dly. To a proper ſelection of medicines, to excite new action in parts leſs eſſential to life, thereby, abſtracting morbid action from vital ones. Theſe then being objects of the greateſt magnitude, it is our duty to adopt every conſideration which may, in the ſmalleſt degree promote their accompliſhment. Nothing, in my opinion, will ſo certainly produce this effect as a well directed attention to the partial operation of medicines, and a judicious arrangement of the different articles of the materia medica according to the ſyſtems, or parts of ſyſtems, upon which they are found to operate. The advantages that would reſult from ſuch an arrangement, over every other hitherto propoſed, are many and important.

1ſt. According to the hitherto adopted diviſion into ſtimulants, aſtringents, tonics, &c. we are incapable of ſelecting with any degree of certainty ſuch medicines as are adapted to the cure of the diſeaſes of a particular ſyſtem. Is a patient labouring under tetanus? It is a common, and I believe a proper practice, to employ ſtimulants in moſt caſes. Now, by what are we to be governed in our ſelection from this numerous claſs? By nothing, as far as I know, but the peculiar whim of the phyſician; hence has ariſen the exhibition of ſuch a vaſt variety of unappropriate medicines for the cure of every diſeaſe.

But, according to the arrangement I have propoſed, we have fixed laws to direct us. The firſt circumſtance to be aſcertained, before we preſcribe for a patient, is the ſyſtem or ſyſtems diſeaſed, and the nature and grade of morbid action; then by referring to that claſs of medicines which is appropriated for the diſeaſes of thoſe parts, which are now affected, we can at once ſelect the proper remedies.

2d. A proper examination of the operation of medicines, upon the ſeparate ſyſtems, is the only proper method of aſcertaining their properties. If, in exhibiting them, we

direct our attention only to their effects upon the whole ſyſtem as one indiviſible machine, their operation upon the different parts may eaſily paſs unnoticed, but by minutely obſerving their action upon the ſeparate ſyſtems, we ſhall diſcover the parts upon which they act ſpecifically, and profit accordingly.

Having finiſhed my intended obſervations upon the *specific* action of medicines particularly, it is neceſſary I ſhould next enquire how they operate, whether through the medium of the ſtomach or the circulating blood.

That all medicines act through the medium of the ſtomach alone, has never, I believe, been aſſerted by any writer, but this opinion is now entertained by ſome phyſicians of eminence. It is my opinion that they operate moſt frequently in this way, but that they are ſometimes abſorbed in their active ſtate, and produce their peculiar effects through the medium of the circulating blood. I ſhall attempt to prove the propriety of my opinion, by the introduction of poſitive facts, which theoretical ſpeculations cannot controvert.—I have heard them denied, although derived from the firſt authority; becauſe, ſimilar facts have not occurred to many other writers. But, the man who offers this denial, acts as

unphiloſophically as I ſhould, were I to deny the exiſtence of any thing, becauſe I never ſaw it. It is an ancient, and true axiom that negative proof is no proof, when oppoſed by poſitive; therefore, one poſitive fact will overbalance a thouſand negative ones. If then the opinion I hold, is ſupported by poſitive proof, and oppoſed only by negative, I ſhall conſider myſelf certainly ſupporting that ſide of the queſtion in which propriety reſides.

It is now my object to prove that medicines have been found in different parts of the body, after having been taken into the ſtomach and paſſed through the circulation.

The great quantity of blood which circulates through the human body, and the ſmall proportionate quantity of medicines which is exhibited, render an attempt to detect them in this fluid difficult, and even impoſſible I believe, in common caſes. The only inſtances in which they can be found here, are when they poſſeſs much colouring principle, or when they have been uſed for a great length of time. Madder tinges the ſerum of the blood of a red colour, and Doctor Preſtwich ſpeaks of a caſe, in which globules of mercury were found in this fluid. As it is much

more eaſy to detect the preſence of medicines in the ſecretions, than in the whole maſs of blood, ſo we have a greater number of inſtances of their being detected here.

Doctor Hamilton of Edinburgh, detected globules of mercury in the milk of a nurſe under ſalivation, by ſlow evaporation. The milk of women frequently poſſeſſes the entire properties of the medicines they have taken. Thus we are credibly informed, that the milk of a nurſe in a ſtate of ſalivation will ſalivate the child, and every old woman knows that cabbage eaten by the mother, will gripe the infant. Profeſſor Barton, mentions ſeveral inſtances of opium, ardent ſpirits, and cathartic medicines, producing their uſual operation upon children, through the medium of the mother's milk, although in ſome the mother was not the leaſt affected. Doctor Percival alſo obſerves, that a purgative given to one who ſuckles, will ſometimes produce no operation upon her bowels, but a powerful one on the child at the breaſt. I am aware it may be urged that the effects produced upon children in the preceding inſtances by the mother's milk, do not ariſe from the preſence of medicines in it, but from their action upon the mother through the medium of the

ſtomach, producing diſordered actions in her ſyſtem, and thereby creating a morbid ſecretion. In ſupport of this idea may be adduced the fact, that, if an angry mother ſuckles her child, it will be often violently affected. To this I would reply, that the fact is no argument againſt the opinion, which ſuppoſes the preſence of medicines in the milk, that were taken into the ſtomach; becauſe in the caſes related by Drs. Barton and Percival, the medicines had no operation upon the mothers, therefore there could be no cauſe for a morbid ſecretion. Beſides, the milk of an angry mother has no determinate effect upon the infant, whereas that of a woman who has taken medicine produces the ſame effects preciſely as the medicine itſelf. Thus, has the mother taken jalap? her child is purged; has ſhe taken opium? her infant is put to ſleep—and if ardent ſpirits, it is intoxicated.

Profeſſor Thunburg, having uſed lead for ſome time internally, perceived it in his ſaliva. Mr. John Hunter, whoſe authority is equal to that of any writer, found all the preparations of mercury ſoluble in the ſaliva, and with a view to aſcertain, whether the ſame taſte would be produced in his mouth by applying them to diſtant parts which he perceived by

holding them there, he made the following experiments. He rubbed mercurial ointment into the internal part of his thighs, until his mouth became affected, when he plainly tafted the mercury. After recovery from the operation of the ointment, he took calomel 'till it produced a falivation; precifely the fame tafte was perceived, as when mercury was held in the mouth.

Thefe experiments plainly prove, that the mercury entered the circulation, and was conveyed to the falivary glands.

Mercury has been found in the bones of perfons who had taken it in large quantity, by Drs. Haller, Mead, Boerhaave, Brodbelt, Boyle, and many others.

Dr. Darwin detected the prefence of nitre in the urine of a perfon to whom he had given it. He however attempts to fhew, that it arrived there without entering the circulation, by a retrogade motion in the abforbents. I fhall endeavour to combat this opinion in a fubfequent part of this effay. I might now go on, and fill a volume with the enumeration of facts fimilar to the preceding; but, believing they are fufficient to prove that medicines are fometimes abforbed into the circulation in their active ftate, I fhall omit them.

I know it has been ſaid that the diſcovery of medicines in the ſecretions, does not prove that they entered the circulation in their active ſtate, but that they were ſo changed by the abſorbent veſſels as not to be abſorbed as medicines, but as the common matter of food, and were recompoſed after leaving the circulation.

This aſſertion, in my opinion, has nothing to recommend it but the ſpeculations of its patrons. It cannot poſſibly be true; becauſe, it is impoſſible to diminiſh the active properties of ſome medicines which have been found in the ſecretions, by any change we are capable of making from their original ſtate. Is it not notorious that mercury is leaſt active in its fluid form, and is it not a fact that every change from this ſtate encreaſes its activity? If then, every alteration in this mineral tends to render it more powerful, ſurely if the abſorbents undertake to change it, they muſt produce a more active medicine. The theory, therefore, of the abſorbent veſſels poſſeſſing a power of changing and deſtroying the properties of medicines, is here directly oppoſed by an impoſſibility.

It has often been objected to the opinion I advocate, that, if medicines were taken into

the circulation in their active ſtate, they would produce death; becauſe, the moſt ſimple ones injected into the blood-veſſels will cauſe this effect. Even admitting that they do produce death when injected into the blood-veſſels (which, however, is contradicted by many late experiments) yet it only proves what is already known, that the internal ſurface of the blood-veſſels is very excitable; that medicines ſuddenly introduced are too powerful for their excitability, and that to be beneficial, they muſt be carried there by the abſorbents, and thus gradually introduced; by which means, the ſtimulus is proportioned to the quantity of excitability. No more occurs here than, caterus paribus, will happen when medicines are applied to other parts of the body. Thus, opium, if taken into the ſtomach in quantity ſuited to the degree of excitability, is an excellent medicine, but if over-proportioned is a deadly poiſon. So it is with injecting medicines into the blood-veſſels; if adapted to the quantity of excitability, they may be beneficial, as I have before ſhewn, but if too powerful they will proſtrate all action. It is therefore plain, that their operation is governed by the ſame laws, whether they be injected into the blood-

veſſels, or whether they be applied to the ſtomach; in either way, they may be rendered ſalutary remedies or fatal poiſons.

Although I have attempted to prove, that medicines are ſometimes abſorbed into the circulation in their active ſtate, and there produce their peculiar effects; yet, as I have before obſerved, I believe they moſt frequently operate through the medium of the ſtomach. I ſhall, therefore, make a few remarks on this proceſs.

The ſtomach, I conſider as the moſt important part of the animal body. This is evinced by its almoſt univerſal preſence in all animals. Many are found to want brain, heart, and other parts eſſential to human exiſtence, but very few are without ſtomachs. The great excitability of this viſcus, the unequal ſympathy which it poſſeſſes with every other part of the body, and the ſervile dependence of the whole ſyſtem upon it, has induced an eminent author to call it the ſeat of the ſoul. Doctor Ruſh, with his uſual ingenuity, compares it to the houſe of repreſentatives. It is certainly the ſpot where the ſyſtems meet, or are all repreſented—the truth of which is evinced, by different medicines producing their *ſpecific* operation upon

each ſyſtem, through the medium of this organ.—Every part of the body however, is not equally repreſented; but, I believe, according to their importance in the animal œconomy. Dr. Cullen aſſerts, that "there is a ſpecial conſent between the ſtomach and the veſſels on the ſurface of the body, ſo that the ſeveral ſtates of theſe are mutually communicated to one another."

By this "ſpecial conſent" and mutual communication of ſtates, he explains the ſweating which attends vomiting. That the ſympathy between the ſtomach and ſkin is great I agree, but that their ſtates are not always alike, is proved by the fact, that ſweating does not always attend vomiting, and that it depends entirely upon the medicine exhibited. Thus, if the vomiting is produced by ſquills or digitalis, it is accompanied by a copious flow of urine: if by the ſulphate of mercury, with an increaſed ſecretion of ſaliva, and if by ipecacuanha or the tartrite of antimony, ſweating. Here we plainly ſee that the Doctor's opinion will not hold good, and that what he ſuppoſed to be the conſequence of a "ſpecial conſent," is only the effect of the *ſpecific* operation of medicines.

That medicines often operate through the

medium of the ſtomach is proved by the fact, that they have been vomited perfectly entire many days after having produced their ſalutary effects. The operation of the bark, in my experiment with it, was over before it could poſſibly have been abſorbed. The effects of medicines are then, often ſo quickly produced in different parts, after being taken into the ſtomach, that no other way is left to account for their ſudden manner of relieving, but on the principle of the repreſentation of every part of the body in this important organ, and of the *ſpecific* operation of medicines. It is in this way, we muſt ſuppoſe the peruvian bark exerts its beneficial influence, in preventing the acceſſion of an intermittent paroxyſm. It is in this way, that opium ſtops the paroxyſm although the cold ſtage has commenced; and it is upon this principle that moſt medicines produce their ſalutary effects upon the human body.

In compliance with my promiſe, I ſhall now take up the conſideration of Dr. Darwin's opinion which ſuppoſes, that, the abſorbent veſſels are capable of taking on a retrogade action, whereby their contents are regurgitated, and that, fluids often paſs in this way from the ſtomach to the bladder.

Believing no ſuch action or communication from one part to another ever to take place, I ſhall endeavour to ſhew, that the Doctor's theory has nothing to recommend it but his viſionary ſpeculations. In doing this, I ſhall briefly conſider ſome facts which have been offered by other writers, and which would ſeem to evince its propriety; without being able to derive aſſiſtance from any perſon.

Dr. Darwin commences his obſervations by deſcribing the Abſorbent Syſtem; his deſcription however is not an accurate one. Every reader muſt plainly perceive that his peculiar partiality for his favourite doctrine, leads him to attribute to theſe veſſels, fewer valves than they really poſſeſs. This diſpoſition is fully exemplified by his quoting authors, who wrote when little was known about this ſyſtem, (becauſe their aſſertions were more coincident with his theory) although they have been contradicted by late experiments. He obſerves, that "many of "the abſorbent veſſels, like ſome of the veins, "are replete with valves—" "Theſe valves "however, do not appear to be neceſſary to "all the abſorbents, any more than to all the "veins, ſince they are not found to exiſt in

" the abſorbent ſyſtem of fiſh, according to " the diſcoveries of the late ingenious and " much lamented Mr. Hewſon!" Our author diſcovers here what I am ſorry to ſee—an improper conſtruction of a writer's words for the ſake of eſtabliſhing a favourite doctrine. His anxious wiſh to bring the abſorbents on a level with the veins as to the number and uniformity of valves, that he may hereafter derive ſome aſſiſtance from analogy, is very obvious—and, although he places them upon the ſame footing, yet he has nothing to ſupport him in ſo doing, but deducing concluſions from the " diſcoveries" of Mr. Hewſon, which this writer's own words will not warrant. The following are Mr. Hewſon's words on this ſubject: " Theſe veſſels (meaning lymphatics) in fiſh either have no " valves or the valves readily give way, for " it is an eaſy matter to fill them contrary " to the courſe of the lymph." Theſe are the words from which Doctor Darwin ſo confidently concludes that the abſorbents, like the veins are not always ſupplied with valves. Even ſuppoſing the abſorbents in fiſh to be without valves, are we to infer that the lymphatics in the human body alſo want them? Surely not—ſuch an inference would be in

the higheſt degree abſurd—becauſe many experiments made before Doctor Darwin wrote, and with which he muſt have been acquainted, poſitively prove that every abſorbent veſſel in the human body is amply ſupplied with valves. But every impartial reader will agree with me in ſaying, that Mr. Hewſon was not certain whether the valves were wanting, or whether they eaſily gave way, as he himſelf expreſſes.

Although this gentleman after his experiments could not determine whether there were valves or not, yet our author who did not ſee the experiments aſſures us, theſe "diſcoveries" prove that the abſorbents like ſome of the veins are without valves!!! The plain and ſimple concluſion however, from Mr. Hewſon's own words is directly oppoſed to that which Doctor Darwin has unwarrantably drawn. The tender fabric of fiſh, neceſſarily implies the ſame ſtructure of lympathics—this plainly accounts for the eaſe with which the veſſels were filled contrary to their valves. This concluſion is poſitively proved by the fact related by the ingenious Mr. Cruikſhank, that the lymphatic veſſels of all animals in which they have been found, are plentifully ſupplied with valves.—This au-

thor alſo remarks, that he ſhould not eaſily believe any veſſel to be a lymphatic, where the characteriſtic of valves was wanting; and he further adds, "The vaſt number of valves "not only diſtinguiſh and characteriſe the ab- "ſorbent veſſels from all others in the human "body, but alſo in quadrupeds, birds, amphi- "bious animals, fiſh of warm blood, as they "are ſtyled, and even in fiſh of cold blood." From experiments he ſays, that only ſome of the veins are ſupplied with valves, and even in thoſe they are not near ſo numerous as in the abſorbent veſſels.

As theſe veſſels are then diſtinguiſhed from all others in ſeveral important particulars, no concluſions drawn from any appearances in other parts bear any relation to what may take place in them. If therefore Doctor Darwin brings forward (as we ſhall ſee) nothing but what he calls analogy in ſupport of this doctrine, it muſt fall unſupported, as there is no analogy to the abſorbents in the human body.

After finiſhing the anatomical part, the Doctor goes on to ſhew that "the valves of "the abſorbent ſyſtem may ſuffer their fluids "to regurgitate inſome diſeaſes." Upon the authority of Doctor Haller, he ſtates that

mercury, ſuet, or water, injected into the lymphatics, or air blown into them paſs eaſily in a contrary direction to their valves, if the veſſels are a little forcibly dilated.

Admitting the fact related by Doctor Haller, what does it prove? Certainly nothing in relation to the action of the abſorbents in a living ſtate. The ſtriking difference between animals in a living and dead ſtate is well known to Doctor Darwin, and that this fact relates only to the latter, is equally plain. To infer from it that fluids may regurgitate in a living ſtate is equally unphiloſophical, as to ſuppoſe the human ſyſtem devoid of ſenſibility and excitability when alive, becauſe it is ſo when dead. It only proves then what every man knows, that if the projectile force with which fluids are thrown into an inanimate tube be greater than the reſiſtance offered by the inequality of its ſurface, they will paſs through it.

I would here however aſk the Doctor, how this quotation accords with his inference from Mr. Hewſon's "diſcoveries"? Becauſe injections paſſed through the lymphatics of fiſh, he concludes that they have no valves, and now attempts to ſhow that they may eaſily paſs through abſorbents which are plen-

tifully ſupplied with valves. The cauſe of theſe peculiar aſſertions is obvious. By Mr. Hewſon he wiſhed to prove that the lymphatics are not always ſupplied with valves, by which means he would place the veins and abſorbents on a level. This would very well ſuit his purpoſe—and by Doctor Haller he attempts to ſhew that valves are no great obſtacles to the regurgitation of fluids. Yet to accompliſh this, he is obliged to draw concluſions from the ſame facts which contradict each other!!!

Our author then relates an experiment which proves, that a bladder when inverted and filled with water, allows it rapidly to paſs out; and from thence infers, "That there is "no obſtacle, at the mouths of theſe veſſels "(lymphatics) to prevent the regurgitation "of their fluids." The ſame objection which were offered to the laſt experiment, apply with equal force to this, as well as to all others, upon dead parts. At any rate the concluſion which is drawn from it, muſt be viewed with aſtoniſhment, by all who have heard of Dr. Darwin's fame. Mr. Cruikſhank's experiments plainly prove, that all abſorbents are plentifully ſupplied with valves

to prevent a regurgitation of their contents.

Beſides, in ſeparating the bladder from the dead animal, a great number of blood-veſſels are neceſſarily ruptured—the reaſonable concluſion therefore is, that the water paſſed out through them.

The Doctor proceeds in his retrogade action, by offering what he calls analogies. He ſays the upper and lower orifices of the ſtomach are cloſed with valves, yet a regurgitation of its contents ſometimes occurs. The colon is ſupplied with valves, which ſometimes allows the ſame action—the like takes place in the lachrymal ſack. And he quotes Dr. Haller who ſays, that a retrograde motion of the blood is obſervable in the veins of animals in the act of dying, from the very heart, to the extremity of the limbs.

I have already ſhewn that what may take place in any other part is no proof of the ſame occuring in the abſorbent veſſels, as they are characteriſed and diſtinguiſhed from all others in the body. The orifices of the ſtomach have no valves although circular muſcular fibres perform their functions by contraction and relaxation. As to the valve of the colon it is only one and that differs ſo

materially from the valves of the lymphatics that it is a matter of diſpute whether it be a valve or only a ſphincter.

The lachrymal ſack has no valves, therefore a regurgitation of its fluid is by no means ſurpriſing.

I am ſorry Dr. Haller does not mention the animal in which this retrograde motion of the blood is obſervable. I am however, induced to believe he has been entirely deceived. The deception aroſe I ſuppoſe from the following circumſtance:

It is well known that the veins of animals in the act of dying become turgid with blood; this appeared to the Doctor I ſuſpect, to be the effect of a retrograde motion: but the fact is eſtabliſhed that the heart and arteries retain life longer than any other parts of the body, and continue to contract until they become nearly empty, thereby filling the veins. As the heart and arteries then continue to act after the death of the veins, it is impoſſible for a regurgitation to take place in them as long as life remains. But even admitting the fact, it does not prove in the ſmalleſt degree that a retrograde motion ever takes place in the abſorbents, becauſe they are uniform-

ly and plentifully ſupplied with valves, and many of the veins have none.

After ſtating the foregoing arguments in favour of a retrograde motion in the lymphatics, the Doctor goes on to prove a communication from the ſtomach to the bladder by means of abſorbent veſſels, and that it is by this retrogade motion that fluids paſs from the former to the latter. In ſupport of this opinion, he firſt adduces the expedition with which cold water and other fluids when drank paſs from the ſtomach to the bladder, and the ſimilarity of the urine when produced in this haſty manner to the material that was drank. That, urine is very quickly formed after drinking, and that, it does reſemble the fluid taken into the ſtomach is true; but does this prove that it gets there by a retrograde motion of the abſorbents? By no means—the certainty of ſuch a communication of veſſels is far from being eſtabliſhed, therefore concluſions drawn from thence are built only upon a ſuppoſed foundation.

No communication of lymphatics from the ſtomach to the bladder has ever been diſcovered in the human body. The only fact which would ſeem to favour this ſuppoſition is derived from Mr. Hewſon who ſays, that

ſome ſmall branches of lymphatics would "*seem*" to join thoſe from the duodenum in birds. But Dr. Darwin has inferred from this obſervation, "anaſtomoſes to be very "frequent between the inteſtinal and urinary "lymphatics as mentioned by Mr. Hewſon." How far the Doctor's inference accords with Mr. Hewſon's remark I leave the reader to determine.

Even ſuppoſing the lymphatics of the ſtomach to join with thoſe from the bladder, ſtill fluids could not paſs from one to the other in a retrograde direction; becauſe the fluids in leaving the ſtomach muſt go in the natural direction of the abſorbents until they arrive to the communication with thoſe from the bladder, when they muſt turn back and go in a retrograde way to get to this organ. That ſuch a proceſs never can take place in the human body muſt be obvious to every impartial reader; for if there is a communication by means of lymphatic veſſels between the ſtomach and the bladder, there muſt be the ſame between the ſtomach and many other parts of the body; conſequently fluids would as frequently paſs to other parts as to the bladder.

The great quantity of blood which is ſent to the kidneys in a given time, readily ex-

plains the early diſcharge of fluids after drinking. Doctor Monro obſerves " the " largeneſs of the renal veſſels demonſtrates " that they cannot receive much leſs than an " eighth part of the blood of the whole body " at a time, and conſequently above one thou- " ſand ounces of blood are conveyed through " the kidneys in an hour, and it will appear " but a moderate allowance for twenty or " even fifty ounces of water to diſtill from " that quantity of blood in the ſame time. " Finally it is certain that both men and brute " animals periſh if the ureters are obſtructed " by a ligature or otherwiſe—we then ob- " ſerve alſo, that no urine can be found in " the bladder.

The ſhorteſt time in which Doctor Darwin diſcovered the fluid in the urine after being taken into the ſtomach, was half an hour, and the quantity eighteen ounces. Now as five hundred ounces of blood paſs through the kidneys in that ſpace of time in common caſes, as we know this may be increaſed by taking certain fluids into the ſtomach, and that the ſecerning action of the kidneys may alſo be much accelerated; that ſome fluids which Doctor Darwin mentions, require very little elaboration to be changed into urine,

and that, part of the eighteen ounces was ſecreted before the fluid entered the circulation —the proceſs of the ſpeedy production of urine after drinking, which the Doctor thinks cannot be explained in any other way but by retrograde motion in the abſorbents, is eaſily diſcovered.

As to the ſubſtances detected in the urine by Doctor Darwin, it accords perfectly with the opinion I have endeavoured to eſtabliſh, which is, that medicines are ſometimes abſorbed into the circulation in their active ſtate, and conſequently may be found in the ſecretions.

The Doctor then quotes the following facts from Doctor Haller, which however he did not ſee himſelf, but were obſerved by preceding writers. Doctor Haller mentions that Doctor Kratzenſtein ſays he put ligatures upon the ureters of a dog, and yet he diſcharged urine. How much? The quantity is not mentioned; we may therefore conclude that it was very ſmall, and that what was evacuated came its uſual way, the ureters not being well ſecured. This concluſion is rendered undeniable by later experiments, wherein the ureters have been tied and no urine diſcharged.

I have already related what Doctor Monro, (whose authority is far more to be depended upon than that of such ancient writers) says upon this subject. Doctor Darwin takes care only to select such facts from Doctor Haller's collection, as bear the face of probability, but when they leap the bounds of possibility as many of them do, he passes over them in silence. To shew how far these facts are to be depended upon, I beg leave to introduce the following quotation from Doctor Haller. In doing this, I do not mean to insinuate that the Doctor himself is not to be believed, but that the facts which he has collected from writers who wrote when medical science consisted in witchcraft, charms, and the like, are not entitled to the least credit. He says upon the authority of others, when speaking of substances passing unchanged into the bladder, "Aliqui etiam ad corpora varia "respexerunt, quæ deglituta in vesicam de-"scendissent, hordei spicas, petroselinum, "herbas varias, brasicam rubram, anisi semen, "pilulas, prunæ, aciculas, officula pilos, "glandes plumbeas, vitri frustum, lolii "spica." It is also said in the same book, that urine has continued to be discharged af-

ter the ſuppuration or total deſtruction of the kidneys.

Knowing how common it is for unuſual occurrences to be exaggerated by thoſe who diſcover them, (and particularly ſo if often communicated by different perſons) and recollecting the ſource from which this fact is derived, we may ſafely conclude that the kidnies were only in a ſtate of ſuppuration or partial deſtruction. Now that the urine ſhould be diſcharged in this inſtance is nothing very ſtrange or uncommon.

It only ſhews what may be ſeen every day, the tranſcendant wiſdom of nature. Do we not often ſee the circulation of the blood in a limb almoſt entirely obſtructed by the deſtruction of the principal artery, and yet this function performed by the ſmall anaſtomoſing veſſels? Are there not caſes wherein the brain has been nearly deſtroyed and yet its functions performed? Yes there are ſuch inſtances upon record, and a friend of mine alſo informs me, he once ſaw a caſe where a patient was diſeaſed for a long time, but the operation of his mind remained perfectly entire, at length he died, when upon opening the brain, at leaſt three-fourths of it was deſtroyed. How often in conſumptive patients are the

lungs found almoſt totally deſtroyed? And laſtly, is it not a law of the animal œconomy that the gradual removal of a part of an organ ſhould increaſe the power of the remainder. Theſe conſiderations plainly evince that a part of the kidnies muſt have remained entire, which continued the ſecretion of urine.

As yet then we are furniſhed with no proof of a retrograde motion ever taking place in the abſorbent ſyſtem. Dr. Darwin however ſuppoſing the fact to be eſtabliſhed, proceeds to account for many phenomena of diſeaſes, by means of it. He aſſerts that the ſudden diminution of the ſize of the abdomen in aſcites attended with an increaſed diſcharge of urine proceeds from this retrograde action in the abſorbents, and accounts for the abſorption of fluids by the ſkin from the atmoſphere in the ſame way!!—Now as there are abſorbent veſſels which ariſe from the ſkin and cavity of the abdomen, and which take up fluids from thoſe parts and carry them into the circulation in their natural direction, to aſſert that they paſs in a contrary way is offering violence to all human underſtanding.

The Doctor mentions ſeveral inſtances of what he calls tranſlation of matter from one

part to another which he ſays is performed by a retrograde action in the lymphatics. Even ſuppoſing matter to be carried from one part to another by the abſorbents, ſurely the rational concluſion would be, that it paſſed in the proper direction of the valves, and not in a contrary way. But the fact is, that what he ſuppoſed to be the tranſlation of matter, was nothing more than a morbid ſecretion of one gland, from the obſtruction of another. This I ſhall render at leaſt probable by anſwering the following queſtion propoſed by him: "Is "it not an immutable law in animal bodies "that each gland can ſecrete no other than its "own proper fluids?" I anſwer no—it is not—and I can prove it by facts and obſervations. There are many inſtances of a ſtoppage of the gonorrhœal diſcharge from the urethra producing the ſame ſecretion from the glands about the eyes. There is a woman in this city who has had her menſtrual ſecretion performed by her ſtomach for ſeveral years, and I have known a caſe, where this fluid has been ſecreted by the breaſt for two years. Theſe are plain facts which evince that a gland may not only ſecrete a fluid in diſeaſe entirely different from what it ſecretes in health, but may take on the ſecretion of another gland.

Having now noticed the principal arguments offered by Dr. Darwin in favour of the retrograde action of the abſorbents, I ſhall very briefly conſider a caſe related by Dr. Senter of Rhode Iſland* which would ſeem to eſtabliſh that theory. This caſe was in a young woman who was attacked with cough, difficulty of breathing and fever, which was ſucceeded by a ſuppreſſion of urine and a diſcharge of pus from the ſtomach. This continued for three years during which time, if her urine was not drawn off by the catheter ſhe frequently diſcharged a ſimilar fluid from her ſtomach.

This with ſome ſimilar caſes would ſeem to evince a direct communication between the ſtomach and the bladder, but upon a minute examination we ſhall find, that the circumſtance may be eaſily explained upon the known laws of the animal œconomy.

The explanation of the fact I ſuppoſe to be in this way; as the urine could not be diſcharged through the natural outlet from the bladder, the abſorbents which ariſe from its internal ſurface were excited into action, whereby it was taken up and conveyed by them into the circulation. As it is neceſſary

* See Tranſactions of Coll. Phys. of Phil.

for the continuance of human life, that this fluid ſhould be diſcharged from the blood, as it is a fact that if the action of one gland be obſtructed, another by ſympathiſing with it will often perform its functions, and as the ſtomach is the great ſympathiſer in the body, the plain and proper inference is, that the fluid diſcharged from the ſtomach was a morbid ſecretion of that viſcus. That the lymphatics of the bladder ſhould abſorb the urine in this caſe, is not an unuſual circumſtance, this takes place more or leſs always when the urine is long retained. If the duct which conveys the bile from the liver to the duodenum becomes obſtructed, the bile is abſorbed and jaundice produced.—That, the ſecretions of a patient labouring under jaundice, all partake of the nature of the bile cannot be denied, and that the ſame thing ſhould occur when urine is abſorbed in large quantities is the natural conſequence.

I now conclude this imperfect Eſſay, and although I believe Doctor Darwin* has theo-

* The ſpeculations which I have attempted to controvert, appeared originally under the ſignature of Charles Darwin, but as they are introduced into the Zoonomia, and are there adopted and ſupported, I have addreſſed myſelf particularly to the author of that work.

rized too far without the ſupport of facts, yet I perfectly agree with him in the following obſervations: "There are ſome modern practitioners who declaim againſt medical theory in general, not conſidering that to think is to theoriſe; and that no one can direct a method of cure to a perſon labouring under diſeaſe, without thinking, that is, without theorizing: and happy therefore is the patient, whoſe phyſician poſeſſes the beſt theory."

It remains now that I return my thanks to the Medical Profeſſors of this Univerſity for the valuable inſtruction I have received from them. To Profeſſor Barton, the American Linnæus, I beg leave in a particular manner to addreſs myſelf, and to ſolicit an acceptance of my grateful acknowledgements.

Wiſhing each of you, Gentlemen, that honour and happineſs which your indefatigable exertions in the diffuſion of knowledge ſo juſtly claim, I bid you Adieu.

FINIS.